



Philosophical Transactions

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IV. Experiments about the Motion of Pendulums in Vacuo. By the Reverend Mr. W. Derham, F. R. S.

I Have long desired to know, whether there be any, or what Difference, between the *Vibration of Pendulums in Vacuo*, and the *Air*. And therefore have divers times recommended the Experiment to many ingenious persons, who had better opportunities of trying it than I. And at last, having seen *Mr Hawkesbee's* Experiments in his Pneumatick Engines shewn before the R. Society, I accomplish'd my desire, by recommending the Experiment to him; who readily undertook it. And having provided himself with a proper Receiver, and all other things necessary, he, with a Friend of mine in London, made the Experiment. The Movements he tryed with were an Eight day Clock vibrating seconds, and an Half-seconds Movement of mine, which I shall speak more of presently. The issue of their Experiment was, My Pendulum vibrated two tenths of an Inch on each side farther *in Vacuo*, than it did in the Free Air, and went 7 seconds slower in 20 minutes, than the other Movement. But in the open Air, my Pendulum in 20 minutes, went only 3 $\frac{1}{2}$ seconds slower than the other Pendulum.

This Experiment (although I distrusted neither the Skill, or Fidelity of the Experimenters) I was desirous to try over my self. And by *Mr Hawkesbee's* generous kindness, in lending me one of his *Air-Pumps* (which I shall always gratefully acknowledge) I was soon furnished with proper Instruments. Which I shall give some account of, that others may judge how far the Experiment

may be relied on, or that it may be better tryed, if mine be found defective.

The first Instrument was an *Air-Pump* of *Mr Hawkesbee's*, a person meritoriously known to the R. Society, who I doubt not is the best Maker of those kinds of Instruments in the World; and his Air-Pumps exceed others in the Ease of being pumped, and in the Certainty of their Evacuation; which may be known to a great nicety.

The next Instrument was the aforesaid *Small Movement*, with a Pendulum of about 10 inches, that vibrateth *Half-seconds*, and is driven by the Power of a Spring. This Instrument I thought commodious, not only for being easily fitted with a Receiver, but also for vibrating Half-seconds very nicely, and also because its Vibrations are equal, not some large, some shorter.

The last Instrument was a very well regulated *Month-piece*, that vibrateth *Seconds* all the Year, with as much exactness as most do.

Being thus accoutered, the Result of many repeated Experiments, day after day, was, That (as before) in *Vacuo* the Vibrations were always larger, than in the Receiver unexhausted. At the first, when my little Movement was newly cleaned, the Vibrations were above $\frac{1}{10}$ of an Inch larger than in the free Air. But afterwards (I suppose from some of the fouled Oyl of the Pump spirtled on the Wheels, in letting in the Air, whereby the force of the Spring on the Pallets was blunted, from hence I say) as the Vibrations in the unexhausted Receiver were a little contracted, so in the Receiver exhausted they were more contracted, and only about 0, 25 of an inch larger than in the Free Air.

The *Alteration in Time*, which this difference of the Vibrations produced, was constantly only about 2 seconds in an Hour slower, in the Receiver exhausted, than in it unexhausted. For if in 4, 5 or more hours going, the two Pendulums did not vary a quarter of a second in the open Air,

Air, or when the Receiver was put over the little Movement, (but unexhausted;) yet when the Receiver was exhausted, the Half-seconds Movement would lose, at the rate of two seconds in every hour, in every Experiment, in many hours going.

And because I was minded to see what alterations would arise from varying the Vibrations, therefore by opening and shutting the Pallets, I caused the Vibrations in some Experiments to be as large as the Receiver would bear; in others, to be as short as possible; always adjusting the Pendulum to vibrate Half-seconds nicely in the Air. But still the success was much the same, or the difference scarce perceptible. But only I imagined when the Pendulum vibrated but a little way from the Perpendicular, that the Vibrations *in Vacuo* were not so much enlarged, as when it vibrated in a larger Arch.

In all these Experiments (which were repeated divers times with the same success) I had no reason to move me to think, but that the Vibrations were enlarged *in Vacuo* by the vast Rarefaction of the *Medium*: I had I say no reason but this, That perhaps the different state of the Air might alter the force of the Spring, which drove the Movement. For the tryal of this, I put a well adjusted *Pocket-Watch* (with *Hook's* Regulator, i. e. the common small Spiral Spring to the Ballance) into the *Vacuum*. And after several tryals, at the same pitch of the Spring, I found not the least alteration in the Watches going in many hours; neither the Springs, nor any other part of the Watch seeming to be in the least affected by the *Vacuum*: but the Ballance circumvolving, or keeping the same Turns, as in the open Air.

But to be still more sure, if possible; it came after this into my thoughts, to try what the success would be by putting the Half-seconds Pendulum again into the Receiver, and only pumping out a part of the Air. And accordingly I left no more Air in, than what kept the included Mercurial Gauge

Gauge at about 6 inches height. The event of which was, That the Vibrations were then not above $\frac{1}{3}$ inch larger on each side, than in the Receiver unexhausted: and the Time lost, but about half a second in an hour, or $\frac{1}{4}$ at most. And so, according as the Mercurial Gauge was more or less high, I always found the Vibrations greater or lesser; they gradually decreasing, according to the quantity of Air re-admitted.

Having thus given an account of what I experimented, with all the Variety which I thought necessary, and with all the care I could, I shall for a Conclusion make some Remarks, and draw some Inferences from what hath been said.

1. What Mr *Foyl* long since observed (from a Cock'd Pistol going down as fiercely in his *Vacuum*, as in the Air) may be hereby farther confirmed, *viz.* That the Air is not the Cause of the Motion of Restitution in Solid Bodies, as Springs. For if it was, it would certainly have been discovered in so tender an Instrument as a well adjusted Pocket-Watch, lying under the perpetual Influence of two Springs.

2. As in *Vacuo* (where the pressure of the Atmosphere is taken off) Heavy Bodies descend quicker than they do in the open Air. So it may be observed, That Pendulums move swifter in the Receiver exhausted, than in it unexhausted.

That Heavy Bodies descend quicker in *Vacuo*, is evident from the swift Descent of the less heavy Bodies, as *Cork*, the *Down of Sowthifiles*, the *lightest Feather*, &c. which do all precipitantly descend, like a Stone, in a tall exhausted Receiver.

And that the *Pendulum*, in our Experiment, moved faster in *Vacuo*, is manifest from its vibrating but two seconds in an hour slower, when the Vibrations were $\frac{1}{3}$ of an inch on a side, enlarged by the highest Rarefaction of the Air. Whereas I find by experiment, that near the same increase of the Vibrations, doth, in the open Air, make the Pendulum go 6 or more seconds slower in an hour. I say *near the same increase*, because it is scarce possible to manage the Pallets so, as nicely to make the same Vibrations as were in *Vacuo*.

The last thing I shall deduce shall be by way of Querrey viz. Whether the Variations of Pendulums observed under the Equinoctial, and between the Tropicks, do not arise as much or more from the rarity of the *Medium*, and the encrease of the Vibrations consequent thereupon? It is scarce I think to be doubted, but that the Air is much thinner, and finer near the Line, than it is without the Tropicks. And it is evident from the Barometer, that on the tops of high Mountains the Atmosphere gravitateth less than nearer the Center. And therefore (although I like the Notion of the Decrease of Gravity from the Encrease of the distance from the Earth's Center; I say, although I like this too well to discard it, yet) I am apt to think that this is not the only reason of the *Phænomenon.*

I wish therefore that *Captain Halley*, when he observed at *St Helena* his *Clocks* to go slower than in *England*, had at the same observed whether the Vibrations were not enlarged. It might be theretore very well worth the while for such as have opportunity, to take notice, Whether their Pendulums between the Tropicks do not make larger Arches than in higher Latitudes? Also in what Latitude they begin to alter? Whether the Vibration be greater near the Line, than in any other part between the Tropicks? Or whether the greatest encrease be not always in those places where the Sun passeth their *Zenith*? If the Vibrations be found larger under the Line, or in any other part of the Torrid Zone, then it may be observed, how much larger they are, and in what proportion they encrease, or decrease, by approaching nearer unto, or receding from the place of their greatest Encrease?

Also it may be worth observing, Whether Pendulums do not vary on the tops of high places, or in different states of the Atmosphere, according as the *Mercury* is high or low in the Barometer? But then in this, and indeed in the former cases, it is necessary, or at least very expedient, that the Movement be so exactly well made, that the Power, whether Weight or Spring, do at all times exert the very same force upon the Pads or Pallets. Which is very rarely met with. For most Clocks are apt to vibrate sometimes larger, sometimes lesser Arches in the 24 hours, according as the Weight or Spring doth more or less exert its *forces* on the work.

Thus having given the Society the account which they commanded me; if I have not answered their Expectation, yet I have however shewed my Obedience to their Commands; and given a Testimony of my great Veneration for that Ingenious and Learned Body.